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SOME RELATIONS BETWEEN PHILOSOPHY AND THE STUDY OF LANGUAGE[1]

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I

INTELLECTUAL problems are of two distinct but related kinds. They require us to go either from false or doubtful views to those that are well-established as true, or from confused views to those that are relatively clear. The latter task—that of going from confusion to clarity—is central to what Broad has called " critical " philosophy. And critical philosophy includes a vast number of the traditional problems.

The mind-body problem, for instance, when viewed in Descartes' way, is a problem of relating two diverse substances ; but when viewed in Berkeley's way it is one of relating minds and ideas, and when viewed in the behaviouristic way it is one of understanding the functions of the nervous system. Why, then, do we have *one* problem, rather than three independent ones ? Simply because each way of formulating it purports to recognize the legitimate heir, so to speak, of a question that is asked at a less reflective level. Each purports to clarify our conceptions of mind and matter, preserving what can be preserved without confusion—eliminating only what is confused beyond hope of repair. Indeed, the essential question is concerned with how the mind-body question is to be interpreted.

In such a problem we are not describing the world, but are preparing the way for subsequent descriptions. A growth of concepts is in question, rather than a use of old concepts in forming new beliefs. This growth involves a shift in understanding that takes place beneath our words.

Now it is on this account that emphasis on language, in philosophy, becomes so important. The symbol itself is the most accessible point from which a shift in our concepts can

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be kept in view : it serves both to hold up the unclear concept for our attention and to provide a vehicle for expressing the clearer one. By presenting our problem as one of defining a familiar word (or of establishing its usage in some other way) we shall be likely to profit in two ways. First, our definition will readily lead to the crucial question : whether or not the defined meaning can adequately *take the place of the old one*. It will show that growth from this rough concept to that more intelligible one is sought, and not merely some fresh start from an unspecified point of departure. Secondly, the definition will show, in a way that a statement about the " nature " of so and so will not, that the problem involves a clarification of our views rather than an extension of them. And this last point must always be emphasized unless we are to have confusion worse confounded.

2

Let us now consider what kind of linguistic study is most needed in philosophy.

We might at first suppose that our problem is divisible into neatly isolated steps. We might hope to develop a broad, self-contained theory of how words are related to the objects for which they stand—a theory that would lead us to general canons of symbolism, suitable for determining when word-usage is healthy and when it is pathological. Only then, we might suppose, would we be in a position to apply our results to the problems of philosophy ; and we should have the relatively straightforward task of showing which views violate our canons and which do not.

If we reflect for a little, however, I think we must agree that this procedure is impracticable. It pretends that our initial study of the symbolic process can somehow stand above and apart from philosophy—that so long as we are developing *it* we stand on bed rock, whereas the regions of philosophy, as yet remote from us, are treacherous with swamps. And this will scarcely do. In point of fact, the perplexity and unclarity that hamper us in philosophy will appear beforehand, and in a serious form, within our very study of the symbolic process.

For it will be granted, no doubt, that the relation of words to their designata is never a simple thing. It always goes *via* the people who use the words, and is intimately tied up with their thoughts. (I say " thoughts " where Ogden and Richards would say psychological " reference," and Morris would say " interpretant " or " process of taking account of " ; but I

cannot see that these new terms behave very differently from the old one). If we are to develop a clear and adequate theory of the symbolic process, then, we shall immediately face the difficulty of clearing up the terms " thought ", and " object of thought," and this will lead further into the long-discussed epistemological question about how thought and its object are related. I have a great interest in these questions, but when I discuss them I certainly do not feel that I am on bed rock. Consider, for instance, this parallel case from the history of philosophy : Descartes was emphatic in saying that a material substance must be distinguished from a pattern of sense-experiences, whereas Hume was equally emphatic in saying that a material substance could be nothing else than such a pattern. This divergence was largely a consequence of a prior one, which (roughly speaking) was concerned with the symbolic process : Descartes recognized imageless thought and Hume did not. Hence from Hume's point of view, though not at all from Descartes', any attempt to speak of a more-than-sensory material substance would involve a use of words without thoughts (or ideas) and so a use of meaningless words. Now if the two philosophers could have argued the matter out, they would very likely have found this question about meaningfulness their chief issue. Neither, I think, would have allowed the other to suppose that, when speaking about thought and meaning, he suddenly became immune from philosophical difficulties.

Nor is the situation any different when we turn to the issue, now well-worn with controversy, that has attended contemporary positivism. If a sentence is neither analytic, contradictory, nor empirically testable, is it devoid of cognitive meaning ? Since the term " cognitive meaning " is commonly used in a vague, confused way, the answer will depend on how we decide to clarify its use. We can't decide this all at once ; so perhaps we shall hope to proceed like this : we shall take certain sentences that are not testable and whose classification as " cognitive " is in doubt, and compare them first with those that are obviously to be called " cognitive " and then with those that are obviously not to be so called. If the non-testable sentences, on examination, show marked analogies to the obviously cognitive ones, and marked differences from the obviously non-cognitive ones, we may wish to call them " cognitive " as well ; and in the opposite case, we may wish to make the opposite decision. I am not suggesting that these considerations would force us to one use or the other of the term ; but they would be helpful in guiding our decision.

In conducting such an inquiry, will we be on firm ground, safe from philosophical perplexities? I think not. For with what eyes, so to speak, are we to look for the analogies and differences that will be in question? If we look with severely empirical eyes, we may find that the non-testable sentences differ greatly from the obviously cognitive ones. If we look with metaphysical eyes, we may find that these differences are outweighed by analogies—we may find that the non-testable sentences, like the testable ones, reveal a kind of Platonic entity called a proposition, and that we have a power of insight which shows that they may correspond to metaphysical facts just as the testable ones may correspond to scientific facts. So the matter is likely to go on. We shall not be engaged in questions that are prior to philosophy, but shall be in the midst of philosophy itself.

What, then, is to be done? Have we no better alternative than to lift ourselves up by our boot-straps? I think we are not in such a bad situation as we may seem to be, and in explaining why, I shall first try to indicate how we have been led into our seeming difficulty, and then indicate how we may hope to avoid it.

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We have been led to our seeming difficulty by supposing that clarity must be obtained step by step, each successive step being taken with perfect security. Perhaps we can explain our inclination to suppose this by looking, once more, to the philosophy of Descartes. It is typical of the Cartesian approach to reverse our modern sense of justice, and to hold all our ideas guilty until they are proved innocent. In particular, there must be some one belief that can be established as perfectly and beautifully innocent from the start. This belief then turns witness for another, and provides it with a water-tight alibi; and by a series of such steps, we are to insure the innocence of a large body of beliefs.

Most of us have come to distrust this procedure. We have learned that the initial proof of innocence is hard to find, and that even if we should find it, the belief it would establish is not likely to be a strong witness for anything else. Yet the old habits of thought are too much for us, and continue to take us unawares. This may easily happen when we approach philosophy through a theory of language. Unless our linguistic theory can be proved wholly innocent, we feel, our start will be wholly guilty. But we cannot establish this initial innocence. So we

seem to have one criminal providing unreliable evidence about all the others.

I can see only one way out of this difficulty—that of dropping the Cartesian approach altogether, and of holding our ideas innocent —*innocent of unclarity, no less than of falsity*—until they are proved guilty. I say this not to propound a categorical imperative, but to make an ordinary proposal—a proposal which simply emphasizes in philosophy a procedure that we have long taken for granted in science and in daily life.

If we follow this proposal, what bearing will it have on the philosophical importance of a general theory of language ?

It will not, of course, give us a theory of language that stands apart from philosophy as immune from the attacks of an imaginary sceptic. That asks for too much. But it will lead us to see that such a start is not a necessity. All that we need ask for is a conception of the symbolic process which, in relation to our present knowledge, we can tentatively accept. You may not accept the same view that I do, but each of us can then trust to his own view, and follow it where it leads. The interesting problems will arise when one view or the other, as we follow it out, begins to produce difficulties—begins to prove guilty. Only then will there be an occasion for altering or abandoning it.

But when does a view of language lead us into difficulties ? Let us remember that by the above proposal—which suggests that we continue to accept until forced to reject—we shall be accepting not only a view of language but also much else. Most of us will be accepting, for instance, a good part of deductive and inductive logic, and the many beliefs that serve us in everyday life. So if our conceptions of language and meaning lead us, by steps of reasoning that we accept, to consider unintelligible certain views we have believed intelligible, or to consider intelligible those we have believed unintelligible, then we have a difficulty. I think C. I. Lewis encountered such a difficulty when his theory of meaning led him to say that our reference to past events is a special kind of reference to future ones : for one is puzzled to know what " future " could mean if it is to include the past, rather than stand in contrast to it.

In encountering a difficulty, we shall not thereby locate its exact source, or the means of surmounting it. Perhaps we must revise our views of language and meaning ; perhaps we must reconsider our steps of reasoning ; perhaps we must change our views about whether our old remarks were intelligible or unintelligible. But obviously, our theory of meaning will have no privileged place ; it too must now come under closer scrutiny,

and will not merely test, but will stand in need of being tested by, everything else that we accept.

How are we to decide, when a difficulty arises, whether our views about meaning must give way or whether something else must give way instead? I am unable to provide any rules for this, and am inclined to think that we must decide as we go, rather than before we start. In scientific method one normally speaks, on parallel points, of the criteria of "theoretical convenience" or "simplicity" or "adequacy." Although I find these topics fertile in suggestiveness, I do not find it easy to be precise about them. I can only remark that, for philosophical cases, our conviction that a certain view *must* be retained will sometimes vanish when it has been carefully scrutinized; and one can proceed on the hope that, by half-blind trials, he can eliminate in this way one or the other of the views that compete for acceptance.

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The net effect of these remarks is to suggest that a theory of language does not stand outside of philosophy, but must be judged by its philosophical implications. I must now make clearer, however, that I have been referring, by the term "theory of language", only to a *generalized* study of the symbolic process —to a study which is at pains to clarify the key terms of our language-about-language, such as "symbol," "cognitive meaning," "designatum," and "syntactical rule," and which goes on to consider certain principles of the psychology of language, or to erect broad canons for determining when symbolism is successful. Now perhaps some of you are accustomed to use the term "theory of language" to include far more pervasive and detailed issues than this. More specifically—

If we should set up precise syntactical rules for a number of the broader terms of our ordinary speech, as distinct from our language about language, would you say that we were helping to develop a theory of language? Let me illustrate the question by returning to the topic of "time", which affords a convenient example. Is the theory of language concerned with the special syntax of the word "time"? You are, of course, fully privileged to answer in the affirmative; but if you do so you must sharply distinguish the inquiry from the more general one that I have been referring to. It is one thing to consider the meaning of "meaning" and another thing to consider the meaning of "time." It is one thing to *test* our general conclusions about meaning by applying them to the word "time," and another

thing to expect them to *include* a study of all the detailed problems that that particular word may occasion. If the theory of language is to extend beyond the broad distinctions and principles that arise in studying the symbolic process, and is to include an effort to systematize all the terms of our varied discourse, then it will cease to be an approach to philosophy. It will not merely lead to, but will actually include, the pursuit of clarity which I have characterized as typical of critical philosophy itself.

Let me reserve the term " theory of language " for the generalized study of symbolism, as before, and refer to the more detailed one as the " study of specific terms." I should now like to say a little more about how much a study of specific terms may involve.

If we take the conventional problems about time, say, and reformulate them as problems of establishing the syntax of the word " time," we shall, I think, do much to direct our efforts in an economical fashion. But the seeming simplicity of our approach must not deceive us into supposing that the old problems about time are easily settled. The reason is simply this : our syntactical rules are virtually destined to be trivial unless we consider the full set of purposes they are to serve, and the full set of confusions they are calculated to eliminate. Indeed, the only difference between rules of syntax and rules for playing anagrams is that the former have a function which the latter have not. And the syntactical rules will immediately lose their function unless we continue to ask, " Of what use is it to make *this* rule of syntax rather than that ? "

The answer to such a question, so far from being simple, involves considerations of extraordinary complexity. I have never met anyone rash enough to pretend that he fully understood what was involved ; nor shall I myself be so rash. But I should like to indicate, by example, a means that is useful in helping us approach the question.

Suppose that we should make a syntactical rule that excludes the expression " Time can go backwards " as nonsensical. Someone objects, saying that the expression, though false, seems to him intelligible. In defending our rule, we might resort to a familiar reply that runs like this : " When we say that an automobile can go backwards, we refer to a process that takes place in time. So if time were to go backwards, there would have to be a second time, presumably, for the first time to go backwards *in*."

Now the effect of such a reply, as I think you will immediately agree, is to call attention to a spatial metaphor, which we suspect

is causing a confusion, and which may not do so when it is made blatantly manifest. But although this is how we may explain the effect of our reply, in retrospect, the reply itself does not explain this. In itself it is not a diagnosis but a kind of therapy. It is a verbal *device*, a sheer exercise with words, which stimulates our habits of looking to our way of speaking.

In addition to the device I have illustrated—which deliberately talks a greater nonsense to reveal a lesser one—there are many others: there is the device of making insistent use of simple contexts, both familiar and unfamiliar, to prevent a perplexing word from being studied with academic artificiality, or the device of putting seemingly parallel contexts side by side, to accentuate any dissimilarities of function that may attend them, or the device of finding trivial cases that seem to illustrate the same difficulties that we find in more important ones. The great need of such procedures in philosophy is what led Wittgenstein, perhaps, to say that philosophy is not a theory but an *activity*.

If we want a traditional name for these devices, we shall find no more fitting one than the term "dialectic." I do not want to be held responsible, to be sure, for everything that this name may suggest; in particular, I do not want to defend Hegel's logic. And yet much of the traditional dialectic—from Plato's discussion of whether the whole of a universal (rather than a part of it) characterizes a particular, to G. E. Moore's discussion of the naturalistic fallacy—has had the effect, though not always the calculated effect, of bringing to light the hidden complexities of our speech. I have no doubt that a dialectic which deliberately sought this effect would be doubly illuminating.

There is good reason to be suspicious of any kind of rule-making for words, no matter how rigorously its results are formulated, unless such a dialectic precedes it. For it is the dialectic that gives life to our linguistic habits, and dispels the notion that our common speech, which alone preserves our cultural heritage, need only be half-examined, and that somehow we can make a fresh linguistic start. Dialectic is the first step, even if only that, in helping us to see why one syntactical rule is preferable to another. It does not systematize, classify, or in any way theorize about the considerations that are involved. It is only an involved way of saying "Notice this"; it provides evidence without stipulating the use that is to be made of it. But this is an important step if we are not to seek clarity in a vacuum, and it is the only practicable step so long as the criteria of clarity are themselves a subject of controversy.

5

Some of you may find my remarks disappointingly conservative. I have defended a kind of coherence test for a general theory of meaning, and a dialectic for dealing with the study of special terms. These are old, and perhaps you will expect the linguistic approach to philosophy to provide something more exciting. But a note of conservatism, now that the linguistic approach has become well established, is greatly needed. When a movement is looked upon as new it is likely to bring unwarranted confidence, and lead us to dismiss our problems prematurely. We are likely, in our eagerness for a clarity that avoids pseudo-problems, to attain only a pseudo-clarity that avoids problems. Meanwhile the old sources of perplexity remain with us, and if linguistic theory can resolve them, it remains the case that many of them are not yet resolved.

A sense of our continuity with the tradition will serve to remind us how patiently the clarity of our views must be sought. Nor are we to suppose that this clarity can be obtained by a study of language alone, whether general or specific. Confusion springs from all manner of sources—from pretentiousness, vanity, and rhapsodic enthusiasm; from impatience, sterility, and lack of imagination; from excessive tenderness or toughness of mind; and from the all-pervasive drive that is found in the quest for certainty. The history of philosophy has known all of these forces, and none has escaped criticism. I am confident that a careful attention to language will help us to detect them and to allow for them; but I do not expect it to work miracles.

TWO OF THE SENSES OF "PROBABLE"

By J. O. URMSON

1

Consider such statements as

"The probability of heads is $\frac{1}{2}$."

"The probability of a man of eighty living ten more years is low."

"The probability of suicide is higher in peace than in war."

These may be considered as samples of a group of statements typically made in games of chance, in insurance business, in biology, and in quantum physics. They seem to resemble common sense generalizations and invariant scientific laws in

that they are based on the observations of regularity and extrapolate from observed regularity. They differ in that ordinary generalizations assert an unvarying conjunction or functional relationship between characteristics, while this type of probability statement seems to assert that a constant proportion of instances of one characteristic are instances of another characteristic. We may say that both types assert a regularity, but one an unvarying, the other a proportionate regularity.

Now often we do not feel that the observed regularity is sufficiently good ground to generalize from it unreservedly. In such cases instead of generalizing to " all A is B ", we may say " probably on the available evidence all A is B " ; and instead of making a generalization of proportionate regularity we may say " in the light of available data it is probable that the probability of suicide is higher in peace than in war ".

It seems to me, therefore, that we have already clearly here two quite different senses of " probable ". In one case (henceforth called " type one probability statements ") we use the word *in* a generalization to indicate that it asserts a proportional regularity (is a statistical generalization) ; in the other case (henceforth called " type two probability statements ") we use the word *about* a generalization which may itself be a type one probability statement. This seems quite clearly correct to me. But as some (e.g. Keynes) regard all probability statements as being essentially type two, others (e.g. von Wright) regard them all as being essentially type one, I shall devote the rest of this paper to indicating some of the differences between the two kinds of usage. I hope that some of these will raise points which are also of independent interest.

2

The first point I would like to make in favour of the distinction is this. Let G be a generalization which is not completely established. Then we may make the type two statement ; " Probably G ". In such a case we think that it is possible for us to get further evidence in the light of which we shall be able to say " Certainly G " or at least " Very, very probably G ", or else " Certainly not G ", or " Very improbably G ". Generally, the probability of G increases with favourable evidence and decreases or vanishes at zero with unfavourable evidence. But suppose we make a type one statement, e.g. " The probability of five peas in a pod is $\frac{1}{2}$ ". Here further favourable evidence instead of raising the probability confirms that the probability is $\frac{1}{2}$. On the other hand further unfavourable evidence might

show that we had set the probability too low. It might also of course show that we had set the probability too high, or it might indicate that there was no dependable regularity in the number of peas in a pod, no assignable probability of getting five peas at all. To "show that there is no assignable probability at all" does not here, of course, mean the same as "show that the probability is zero". Thus we have here our first tell-tale piece of evidence. *Favourable evidence confirms probabilities in the case of type one probability statements, unfavourable evidence decreases or increases or destroys such probabilities. For type two statements favourable evidence increases, unfavourable evidence decreases, the probability.*

A further sign of the difference between type one and type two probability statements is the difference in the relation in which they stand to their evidence. It is a point which has often been made that if one man says "Probably G" and another says "Probably not G" it is not necessarily the case that one of them is mistaken. For perhaps G is probable on one set of evidence and improbable on another set. Very little reflection is required to see that the question whether G is really probable in itself, apart from questions of evidence, is not a real question. This state of affairs has been commonly signified by the statement "probabilities are relative to evidence" or the statement that "Probably G" is incomplete since probability is a relative notion. People have tended to generalize this view, which seems obviously correct for some usages of "probable", to cover all usages. But this again seems to me to be incorrect. I wish to say that while it is correct for the case of type two probability statements, and possibly some other usages which we are not considering, it is definitely incorrect in the case of type one probability statements. For surely it is no more improper for me to say that the probability of an A being B is P, to assert a proportional regularity, without specifying my evidence for this assertion, than it is improper for me to say "All As are B" without specifying my evidence for *that* statement. To state simply that the chance of throwing six with a die is $\frac{1}{6}$ does not involve an ellipse of the statistics gleaned from gambling dens. However pardonable the mistake of a man who had seen an extraordinary run of sixes would be if he were to assert that the probability of throwing 6 is, say $\frac{1}{4}$, it would be a mistake. There is one and one only absolute probability of throwing six.

I think that an opponent would have to admit that this distinction is at least plausible. But perhaps he still has a kick left in him. Perhaps he would ask me to admit, as I would admit,

that the statement " The probability of a European understanding the word ' waistcoat ' is P " is an example of a type one probability statement. Now, it will be said, it must surely be admitted that the probability of a European understanding the word " waistcoat " relative to the evidence that he is an Englishman is very high, while relative to the evidence that he is a Croatian peasant, it is very low. Thus, it will be said, there is no single correct value for P, no absolute probability; its value must depend, as in other cases, on the evidence. Similarly the probability of a die falling six uppermost will have different values relative to the evidence that it is a true die and relative to the evidence that it is the property of a highly successful professional gambler.

I shall admit the facts alleged, but deny that they have implications which are not consonant with the view which I am putting forward. This is so because an interesting ambiguity of the word " evidence " is involved. To see this, let us compare the following statements.

1. The probability of a European understanding the word " waistcoat " is very high relative to the evidence that he is an adult Englishman.

2. The probability of an adult European understanding the word " waistcoat " is very high relative to the evidence that he is an Englishman.

3. The probability of an Englishman understanding the word " waistcoat " is very high on the evidence that he is an adult.

4. The probability of an adult Englishman understanding the word " waistcoat " is very high.

It is surely clear that these four statements, three of which verbally assert a probability of a different proposition on different evidence, and one apparently asserts an absolute probability, are in fact simply four different ways of formulating one and the same proposition.

Notice also that an essential reason for our belief that this proposition is true is the tacitly assumed evidence which is in our possession that the word " waistcoat " is a common English word. And this is just a disguised way of stating the statistical fact that nearly all Englishmen understand the word " waistcoat ". Knowing that the Europeans in questions are English is no reason for thinking it likely that they will understand the word " waistcoat " without the information that the word " waistcoat " is a common English word. To ask for the probability of a European understanding the word " waistcoat ", first on the evidence that he is English, second on the evidence

that he is a Croatian peasant, is simply to set in an idiomatic fashion two different problems. The evidence, on the other hand, that "waistcoat" is a common English word does not alter the character of the problem, it presents a clue for solving it.

We may perhaps sum up the point which has just been illustrated as follows. There is one type of evidence which does not function as a clue to the value of the probability but which helps to specify the class of events whose probability is in question. In the language of the frequency theorists, it is a part of the specification of the collective or reference class. We may call this specificatory evidence. A statement may always be recast, as I did in my four sentences, to eliminate specificatory evidence, or increase it or alter it without a real change of meaning. Evidence which does serve to determine the probability in question does not further specify the question, and a type one probability statement, though it may be idiomatically expressed to contain specificatory evidence, is complete without the provision of any of this type of evidence, though it would no doubt be unwarranted if we did not possess such evidence.

If what I have called specificatory evidence did help to solve problems of probability it would be a delightful situation, for only a slight literary dexterity is required to manufacture specificatory evidence. If, for example, we want to know the probability of a European living to be a hundred years old, and we are told that we must provide evidence if we want an answer, then, if specificatory evidence will do, we could say "Very well, forget that question. Tell us instead the probability of a man living to be a hundred years old on the evidence that he's a European. Or if it's easier to answer questions with more evidence than that, tell us the probability of an organism living to be a hundred years old on the evidence that it is a vertebrate mammal of the genus *homo sapiens* and a native of Europe."

In terms of this distinction, I can now say that *type one probability statements are only relative to evidence if the evidence is specificatory evidence*. They can therefore always be restated in a form in which evidence is not mentioned. *On the other hand, type two probability statements are relative to evidence of the substantial non-specificatory type*. Incidentally, I think that the failure to recognize these two senses of "evidence" accounts at least partially for the failure of some theorists to recognize the predominantly statistical character of the evidence in the substantial sense for type one probability statements.

They have seen, for example, that in the case of the question "What is the probability of rain within 24 hours" the evidence

that the barometer is falling or rising is highly relevant and is not statistical. They have, however, failed to see that it is relevant simply because it alters the question in which we would naturally be interested either to " What is the probability of rain following a fall of barometric pressure within 24 hours " or to " What is the probability of rain following a rise of barometric pressure within 24 hours ". These are different questions, and, in the main, meteorological statistics are our ultimate means of answering them both.

It is, unfortunately, true that substantial evidence can be introduced as though it were part of the question at issue and not as evidence. I have said that we can reformulate " What is the probability of a European understanding the word ' waistcoat ' on the evidence that he is English? " to eliminate the specificatory evidence by saying " What is the probability of an Englishman understanding the word ' waistcoast '? " But we can also reformulate " What is the probability of an Englishman understanding the word ' waistcoat ' on the evidence that ' waistcoat ' is a common English word? " to read " What is the probability of an Englishman understanding the common English word ' waistcoat '? " But this question and the simple question " What is the probability of an Englishman understanding the word ' waistcoat ' ? " would only specify different questions if we used the phrase " common English word " to distinguish one usage of " waistcoat " from another uncommon usage. And that is clearly not the case. If substantial evidence is stated as part of the question and not as evidence, then, unlike specificatory evidence, it makes no difference to the nature of the question.

I hope I have not over-elaborated this point, but I am very anxious to make it clear because I want to use it in order to make clear the third difference which I find between type one and type two probability statements. I think this is a very interesting difference because it raises a difficult problem which I do not here try to solve.

Let us, then, take first a type one probability statement and state it in a form in which specificatory evidence appears—we have seen that it does not matter either way. Let us ask " Is it probable that rain will fall within 24 hours on the evidence that the glass has fallen one inch within the previous 24 hours ? " Compare with it this question. " Is it probable that all cows are ruminant on the evidence that cows of many ages and many breeds have been examined and found to be ruminant, but no cows have been found which are not ruminant ? " How would

we set about answering the first question, the one about the weather? Does a drop of one inch in 24 hours more frequently, or more likely presage a gale, or rain, or what? we may ask. In order to answer this question we must ask for evidence, for substantial evidence. And this evidence would naturally take the form of meteorological statistics. In other words, we are answering an empirical question by empirical means. But turn now to the second question: "Is it probable that all cows are ruminant on the evidence that all examined cows have been ruminant?". Is this an empirical question? Surely not! If it is, what kind of empirical evidence would you require to solve it? To examine more cows and perhaps find one that is not ruminant, while it would be relevant to the question "are all cows ruminant?" is simply irrelevant to the question "Is it probable that all cows are ruminant on the evidence that all examined cows have been ruminant?" Thus we are led to formulate our third difference between type one and type two probability statements. *Type one probability statements*, while it is true that they cannot be conclusively verified or falsified, *are empirical statements* in the sense that it is the records of ordinary experience which lead us to accept or to reject them. *Type two probability statements*, on the other hand, *are not empirical statements* in this sense. This, of course, leads on to the interesting question "If type two probability statements are not synthetic *a posteriori*, what are they? Are they synthetic *a priori*? Are they analytic? Or are they something quite different again?"

I now come to the fourth and final difference between the two types which I wish to notice. *Type one probability statements*, though the probability may only be given roughly, *can always be given with a precise numerical value without change of character*.

It may not always be practicable to give a reliable figure—our evidence may only be sufficient either to warrant or to verify an approximation. But the procedure of discovery and verification is not in principle altered by giving a numerical value. For example, my statement that the probability of living to be a hundred years old is very low is of the same type and based on the same kind of evidence, though less elaborate evidence, as the precise numerical value which an actuary would be prepared to give. *But type two probability statements are not naturally given a numerical value*. Supposing that I say that the corpuscular theory of light is probably correct. Well, is the probability nine-tenths or ten-elevenths, or, shall we say, are the odds 9—1 or 10—1? And if I give the odds 10—1, who is to prove, and how is he to

prove, that the odds should have been 9—1? I suppose one can bet about almost anything, including the truth of theories. But if bets are made not on the truth, but on the probability, of theories, I, for one, would not care to arbitrate either as to what would be fair odds or as to who is the winner if odds are assigned. Keynes and many others have noticed this fact that some probabilities can reasonably be given a precise numerical value, and others not. That is not a new point. The point I wish to make is that type one statements fall into the numerical class, type two into the non-numerical. The difference has a clear explanation and should not be left, as Keynes left it, as a strange but ultimate fact.

To sum up, I have pointed out four differences between two types of probability statement.

1. Favourable evidence confirms the numerical value of probabilities in the case of type one probability statements, unfavourable evidence decreases or increases or destroys such alleged probabilities. For type two statements favourable evidence increases, unfavourable evidence decreases the probability.

2. Type one probability statements are only relative to evidence if the evidence is specificatory evidence. Type two statements are relative to evidence of the substantial non-specificatory type.

3. Type one probability statements are empirical statements, to be tested by experience. Type two statements are not.

4. Type one probability statements may always be given a numerical value without change of character. Type two statements are not naturally given a numerical value.

To avoid misunderstanding let me add that

(*a*) I have not offered an "analysis" of any kind of probability statement.

(*b*) I have not denied that there are sub-types within the two types of statement which I have distinguished.

(*c*) I have not suggested that there is any ready-made criterion for judging from the verbal form of a probability statement to which type it belongs.

(*d*) I have not suggested that all probability statements fall within one or other of my two types.